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Vol. 5, No. 4

December, 1951

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# Canadian Journal of Psychology

THE JOURNAL OF THE CANADIAN PSYCHOLOGICAL ASSOCIATION

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## CONTENTS

<i>Socio-Economic Status and Predictive Test Scores:</i> W. W. TURNBULL	145
<i>Individual Differences in Dogs: Preliminary Report on the Effects of Early Experience:</i> R. S. CLARKE, W. HERON, M. L. FETHERSTONHAUGH, D. G. FORGAYS, and D. O. HEBB .....	150
<i>Approaches to the Experimental Study of the Rorschach Test:</i> G. A. FERGUSON.....	157
<i>Blood Pressure Response to Repeated Brief Stress in Psychoneurosis: A Study of Adaptation:</i> R. B. MALMO, C. SHAGASS, and R. M. HESLAM .....	167
<i>Book Reviews</i> .....	180

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PUBLISHED QUARTERLY

MARCH - JUNE - SEPTEMBER - DECEMBER

THE UNIVERSITY OF TORONTO PRESS

\$3.00 PER YEAR

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# Canadian Journal of Psychology

## SOCIO-ECONOMIC STATUS AND PREDICTIVE TEST SCORES<sup>1</sup>

W. W. TURNBULL

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FOR A GREAT MANY YEARS it has been a matter of common observation that different cultural groups perform differently on psychological tests. The first large-scale demonstrations of the differences led some psychologists to conclude that there were at hand the tools necessary to permit comparisons of mental capacity from one cultural group to another. The classic statement of this view is contained in Brigham's *Study of American Intelligence* (1), published in 1923.

More sober reflection followed. The sampling within the various groups was questioned, and the objection was raised that tests developed in one culture are inappropriate bases for comparisons of groups whose customs, traditions, perhaps language, are in great or small degree dissimilar to those of the culture that produced the test.

The obvious solution was to develop "culture-free" tests, several of which have appeared in the last twenty-five years, and which continue to appear. Ingenious though their authors have been, however, these tests continue to draw the criticism that they fail to eliminate the most subtle and pervasive cultural influences that obscure true capacity.

From time to time there have appeared those who urged that, in the absence of conclusive evidence as to differences in the true underlying capacities of racial or cultural groups, we adopt the null hypothesis and assume that no such differences exist. We then have a simple criterion for the adequacy of a test to be applied to different groups. A "good" test will show no differences.

All of this poses two closely related problems. They are: (1) are there true differences in the abilities of different groups, and (2) how can tests be designed to reflect accurately these ability differences?

Unfortunately, all of the approaches to the questions cited so far involve some degree of circularity. Either they assume that the test is accurate and that therefore the test score differences provide an accurate picture of underlying ability differences, or they assume that the underlying ability differences are of X, Y, or Z magnitude and that the problem

<sup>1</sup>Paper given at the Annual Meeting of the Canadian Psychological Association, London, Ontario, on May 28th to 30th, 1951.

is to build a test that will elicit group differences in scores of this same preconceived magnitude. They introduce an untestable assumption, then, either about the magnitude of the difference at issue or about the accuracy of the test used to detect the difference. How can we break out of this seemingly closed system?

There is a simple technique or viewpoint that offers a solution in a large proportion of instances in which the problem arises, specifically those instances involving the use of a test to predict behaviour. The fundamental step in this technique is the familiar but oft-neglected one of defining in behavioural terms the criterion to be predicted. Once this has been done the suitability of any proposed predictive test is readily determined; one merely correlates the test scores with the criterion measure. The appropriate or fair test is the one that yields a high correlation with the criterion and that provides for each group an ability estimate corresponding to its performance on the criterion. The excellence of the test is defined, not in terms of the degree to which the scores of cultural sub-groups conform to one's *a priori* assumptions, but in terms of the degree to which these scores conform with performance on the criterion. The touchstone is not an assumed quantity of inborn capacity but a demonstrated, functional, present or future ability which the test is assumed to predict.

Lest this approach be termed too obvious to need separate emphasis, I should like to mention the recent work of Professor Allison Davis and his colleagues at the University of Chicago. Davis has been concerned particularly with the fairness of mental tests for children of different socio-economic levels. His argument is summarized in the following quotation from an article (3) written by Davis and Havighurst in 1948:

There is no evidence, and no theory shared by the leading human geneticists, to the effect that the under-privileged socio-economic or racial groups are genetically inferior to the more prosperous socio-economic groups. There is abundant scientific evidence, advanced by sociologists and social anthropologists during the past twenty-five years, that a child's particular social and cultural environment directs, trains, and motivates his behavior. Thus, any differences between the average response of different cultural groups to a mental problem may be attributed to their unlike cultures. Therefore, all problems that show socio-economic differences in performance should be ruled out of tests as unfair.

By judicious selection of test questions in accordance with his assumptions that there should be no difference in performance of groups of high and low socio-economic status, Davis has prepared new tests of his own to rectify the error into which he believes all other test-makers have fallen. His findings are summarized in the following quotation from an article (4) which appeared in January, 1951:

In view of the results obtained with the standard tests, we were ourselves not anticipating the results which have come in from the new tests. On one new experimental test, the children from the lower socio-economic white group earned slightly higher average scores at age six than did the higher socio-economic white group, and equalled the performance of the upper socio-economic white group at each of the other three ages.

When one controls the socio-economic cultural factors in a test, therefore, one finds sound statistical evidence that the average real intellectual ability (or what Binet called "capacity" as contrasted to "information") is in general at the same level for all socio-economic groups.

We have come full circle. Davis has assumed a zero difference in ability between socio-economic groups, has built tests specifically to yield a zero difference, and has used his results as evidence that there *was* no real difference, after all; when you eliminate the "unfair" items, the groups achieve similar test scores.

Now let us consider the same problem from the standpoint of the criterion the tests are being used to predict. The use that Davis especially attacks is in the prediction of success in school. His thesis is that the academic ability of underprivileged children is underestimated by present tests. But he checks this hypothesis, not by referring to performance on the criterion, but by looking back to an assumed genetic equality.

If Davis's position were correct, and tests were underestimating the scholastic ability of underprivileged children, one would expect to find that these children made better school records than the tests predicted. This situation would result in a *negative* partial correlation between socio-economic status and scholastic success, holding intelligence test scores constant. What do we find?

There are several studies that provide direct evidence on this point. Three of these may be cited. W. D. Lewis (5) chose some 4,500 children who constituted the upper 10 per cent in intelligence of approximately 45,000 elementary school pupils in thirty-six states. From these 4,500 children, all of whom were gifted according to their test scores, he chose two sub-groups: 1,078 children whose educational age was a year or more above their mental age, and 756 whose educational age was a year or more below their mental age. Lewis then compared the home backgrounds of these two sub-groups and found that the accelerated children came from homes that provided more cultural advantages. It must be remembered that both the accelerated and the retarded children were from the top 10 per cent in intelligence. In other words, intelligence has been held constant experimentally rather than statistically, and we are left with a *positive* residual correlation between socio-

economic status and educational attainment. To put it still another way, the test overestimated rather than underestimated the scholastic success of the underprivileged children in Lewis's group.

A second pertinent group of studies has been made by Stroud (6) and his students at the University of Iowa. In these investigations, correlations were obtained among socio-economic status, test intelligence, and scholastic achievement in elementary grades. Typically the socio-economic measure correlated significantly with both intelligence and academic achievement, but in no case did the intelligence measure *underestimate* the achievement of the underprivileged group. The correlation of socio-economic status with school success, when intelligence was held constant, in all instances remained *positive*.

The third set of studies was conducted by Conrad and Robbins (2), who investigated the college achievement of students for whom they had scholastic aptitude test scores and measures of educational handicap. For the latter measure these authors used the average teacher's salary in the school system from which the individual came. Conrad and Robbins found that the students from school systems where lower salary scales prevailed did indeed make lower test scores than students from schools that could afford higher salaries for their teachers; but the students from the low-salary schools also did less well in college. Whatever educational handicap was reflected in the aptitude test scores received before college entrance was reflected no less strongly in college achievement.

Professor Stroud has written (6) a conclusion for his own studies that can serve equally well to sum up the several investigations I have cited and the general finding reported in the literature:

The conclusion is that the pupils who come from the underprivileged classes and who have a given intelligence-quotient level do not learn more and do not otherwise get on better in school than do pupils who come from the privileged classes and have the same intelligence-quotient level. . . . It is suggested therefore that, for purposes of prediction of success in schools as now organized, intelligence tests appraise the ability of unfavored groups as fairly as they appraise the ability of the average or the favored groups and that, although the low average intelligence-quotient of the unfavored groups may be the fault of society or of biology, it is not due to unfairness inherent in the intelligence tests.

These results, while important in themselves, have greater significance in terms of a general method or approach. In sum, that method is to define the criterion to which a test is intended to relate, and then to justify inter-group equality or inequality of test scores on the basis of its effect on prediction. It is necessarily true that an equality of test scores that would signify fairness of measurement for one criterion on

which cultural groups performed alike, would signify unfairness for another criterion on which group performance differed. Fairness, like its amoral brother, validity, resides not in tests or test scores but in the relation of test scores to criteria.

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#### A.B.E.P.P. CANADIAN AWARDS

The American Board of Examiners in Professional Psychology, Inc., is honoured to announce the award of its diploma to the following nine members of the Canadian Psychological Association in the indicated professional specialties. These diplomas have been awarded to senior members in professional fields of psychology with full waiver of written and oral examinations on the basis of a review of individual qualifications:

<i>Name</i>	<i>Field of Award</i>
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Laycock, Samuel R.	Clinical Psychology
Parmenter, Morgan D.	Counseling & Guidance
Shevenell, Raymond H.	Clinical Psychology
Wees, W. R.	Industrial Psychology

# INDIVIDUAL DIFFERENCES IN DOGS: PRELIMINARY REPORT ON THE EFFECTS OF EARLY EXPERIENCE<sup>1</sup>

R. S. CLARKE, W. HERON, M. L. FETHERSTONHAUGH, D. G. FORGAYS,<sup>2</sup>

and D. O. HEBB

*McGill University*

WE REPORT here the preliminary results of a study of temperamental and intellectual differences among dogs. The development of methods for the study of emotion, motivation, problem-solving, and differences of capacity for learning motor skills is a logically prior objective, important in itself because of the opportunities it would create for a number of useful investigations. But it is probably chimerical to think of first devising adequate methods, and only then beginning research. We have therefore attempted to combine our search for methods with a first study of the effects of the infant environment upon adult behaviour, as one source of individual differences.

The starting point was the earlier study of the rat, made in this laboratory by Hymovitch (4) and Forgays (1) who showed that the animal's opportunities for exploration and complex visual experience during growth were closely related to its problem-solving at maturity. This does not, of course, deny the importance of heredity in intelligence. It indicates a role of the environment in developing the inherited, *potential* capacity for problem-solving. Such a result is not an isolated one, unrelated to other aspects of behaviour, nor peculiar to a single species. A good deal of work in the last decade or two, summarized by Hebb (2, 109-20), indicates that the whole question of the relationship of adult behaviour to the infant environment should be examined. For such a purpose the dog has great advantages. He has a relatively short period of growth, permitting experimental control of the environment during growth, which would hardly be practicable during the five to ten years' period required by monkey or ape; yet he has a complexity of temperament and of intelligence that the rat does not have.

## SUBJECTS AND REARING

A litter of six animals of an inbred Scottish terrier strain from the Jackson Memorial Laboratory was divided into two groups. Three pups, two males and one female, were raised as pets (two in the laboratory,

<sup>1</sup>This research was supported in part by grants from the Defence Research Board, National Research Council of Canada, and the Dominion-Provincial Mental Health Project.

<sup>2</sup>Now at Western Michigan College.

one in a private home), and had considerable handling. The rest, all males, were brought up together in a cage 3 by 6 feet which was specially designed so that the dogs could not see outside, but which admitted light. The dogs were never removed from the cage during this time, and their only contact with humans occurred during the daily feeding and cleaning period. At the age of  $7\frac{1}{2}$  months they were removed, and with their littermates were allowed the run of a large room in the laboratory. At this time there were no noticeable differences in weight or general health between the two groups.

#### TEMPERAMENTAL DIFFERENCES

Peculiarities in the behaviour of the restricted (cage-reared) animals were very marked for the first few days after removal from isolation. They would not go through a doorway without coaxing, and though they appeared to be eager for human attention, they strongly avoided handling. When an observer extended his hand to them or otherwise attracted their attention, they would approach and vigorously lick his hand or feet; but at the first attempt to pat or grasp one of them, the whole group would draw back sharply.

The most striking peculiarity on the part of the restricted animals was the "freezing" behaviour which they displayed when they were placed in unfamiliar surroundings, or handled by an experimenter. The animal would hug the floor, forelegs apart, ears back, and eyes staring forward. This state was apparently accompanied by some degree of anaesthesia; when subjected to injections, for example, the restricted animals "froze" and made no protest when the needle entered. The animals who were reared as pets (free-environment), on the contrary, objected vociferously to the whole procedure.

It was possible, for three or four months, to observe gross differences of responses to human attention between the two groups. For example, if the dogs were kept in cages overnight, the first experimenter to open the cage doors in the morning would find that the free-environment dogs would approach him and be quite eager to be allowed out of the cages. The restricted animals, on the other hand, would withdraw to the back of the cage, and it was difficult to get them out. This avoidance behaviour took place even when both groups of dogs were present in the same cage.

Some of the peculiarities of behaviour on the part of the restricted animals were greatly diminished after about a week; at the time of writing (six months later) they can be elicited under certain conditions, but only in a vestigial form.

When the dogs were 10 months old,  $2\frac{1}{2}$  months after the restricted group had been removed from isolation, a more systematic study of temperament and social behaviour was undertaken. Test situations were devised in which a quantitative description of the results was possible.

In the first test, a competitive situation (fighting for a bone) in which each dog was compared with every other dog, all of the restricted animals were markedly subordinate to all the free-environment animals. When pitted against each other, two restricted dogs would show a tendency to "share" the bone, both gnawing at it for some time before the less dominant animal was driven away. This type of behaviour was never noticed in the free-environment dog, who, when dominant, would immediately drive off the other contending animal.

When two dogs, one from each group, were placed together in an environment with which they were unfamiliar, a "following" score was recorded. "Following" either is defined literally as the running of one dog behind the other, or, when two animals move together side by side, means that one dog (the leader) initiates movement and changes of direction. In much of the activity there was no obvious leader or follower; but where following could be determined, the mean number of "follows" by the restricted group was 12 (range: 7-16), while that of the free-environment animals was 3 (range: 1-6). This difference between the groups, however, was not observed when the animals were in familiar surroundings; that is, the room in which they were normally kept when not undergoing testing, or a room with which they had become familiar because of its frequent use as a testing room.

A test of reaction to unfamiliar dogs was carried out in an area 20 by 40 feet, in one section of which there was a wire pen 8 by 8 feet. A strange dog was placed in this pen, and two experimental animals, one from each group, were brought into the main test area. A record was kept of the number of times each experimental animal went over to the pen and "paid attention to" the test dog inside, and also of the amount of time so spent. The criteria of paying attention were pausing and looking in the direction of the test dog, and more marked social responses such as barking, rolling over, and trying to enter the pen. After five minutes, the strange dog was let out into the main test area, and a record kept of the amount of time that each experimental animal showed interest in it by sniffing, barking at, or rolling over in front of it. This observation period lasted five minutes. The whole test was repeated four times, using a different test animal on each occasion.

The restricted animals were very indifferent to the test dog, and on the whole tended to ignore it, while the free-environment dogs were highly responsive. The restricted dogs went, on the average, 8 times

(range: 4-12) to the pen, spending there a mean time of 44 seconds (range: 9-77); the free-environment dogs went an average of 22 times (range: 13-30) and spent there a mean time of 271 seconds (range: 136-368). In addition, the free-environment animals spent about four times as much time displaying interest in the test dog when it was released from the pen. This behaviour is consistent with what was displayed when both groups were placed in the same room for the first time after the restricted animals had been removed from their cage. Then, the cage-reared animals tended to avoid the free-environment dogs, who on the contrary displayed a lively interest in the cage-reared, sniffing at and following them.

An attempt was made to investigate the reaction of both groups to a mild stress situation by using a modification of a test described in the Bar Harbor Manual (5), which involves a certain degree of hunger frustration. The animals, deprived of food for 24 hours, were placed two at a time in a wire enclosure 8 by 15 feet. They were observed for five minutes, and then a dish of food was placed outside the pen where it was visible to the experimental animals. At the end of another five-minute interval, another dog was allowed to eat the food for one minute, and was then removed. This was followed by a final five-minute observation period. Scoring was on the basis described in the manual, a check being placed every 30 seconds in the column of the score sheet which most aptly described the animals' behaviour at that particular moment, such as sitting, jumping, walking, and so on.

The free-environment animals showed a consistently higher level of activity throughout the test, and made more attempts to get out of the pen. One interesting result was that as the situation became more stressful (when the food dish was placed outside the pen) the activity of the restricted decreased, while that of the free-environment animals increased.

The final temperament test was one that investigated another aspect of motivation. The amount of time each dog spent sitting and lying down under various conditions was recorded. It was found that in unfamiliar rooms, generally, there was no difference in the level of activity between the groups. If taken into an unfamiliar room repeatedly, however, there was a steeper falling-off in the activity level of the free-environment animals. These dogs spent a significantly greater amount of time sitting and lying down after a number of exposures to the room than did the restricted. A parallel observation is that the restricted dogs would continue to tear up and play with or chew sheets of newspaper for a longer period of time than would the normally reared animals.

One other noticeable difference between the groups was that when the animals were taken outside for "intelligence testing," as described

in the following section, the restricted dogs soon learned to run directly to the testing area ahead of the experimenters. The free-environment animals, on the other hand, very often would run off in some other direction, galloping vigorously about. The experimenters would occasionally have considerable difficulty getting them into the test area. These observations indicate differences in stimulation-value with the repetition of a relatively simple situation (or conversely, differences in capacity for "boredom").

#### INTELLECTUAL DIFFERENCES

Four days after the restricted group was first brought out into the wider environment, both groups of animals were given a "barrier" umweg-type test. A series of maze problems and a motor learning test followed between the fourth and eighth weeks, and a "ramp" umweg-type test at nine weeks.

The barrier test apparatus consisted of three sides of a 4-foot square, 2 feet high, with wooden side walls and a wire mesh front. The animal was placed in this, and fed three morsels of food under the wire. On the five test trials food was placed 6 inches outside the wire, and a correct run was scored if the dog ran around the sides of the apparatus directly to the food without attempting to get through the mesh barrier. The time of each trial was recorded.

None of the restricted group made a single correct run on this problem. The free-environment animals averaged 3 correct solutions, with a range of 1 to 5. Moreover, the responses of the former group were more stereotyped; though it was possible to reach food by going round either the right or left side of the barrier, only one restricted dog changed its direction during the five test trials, and then only once. The free-environment animals all changed their direction on at least one run, the mean number of changes for the group being 2. The restricted group spent a much longer period of time per run than the free-environment group.

The most elaborate test used was a modification of the "closed-field" test (3) originally designed to measure intelligence in the rat. For the present study, it was set up outdoors in a space 15 by 26 feet, and the problems used were more complex than those used in testing the rat. The dogs were given preliminary training on 11 problems, with 8 trials on each. The test series consisted of 18 problems, given at the rate of one per day. Errors were defined as entry into predetermined error zones.

The mean error score for the restricted animals was 309 (range: 305-313), and for the free-environment group 236 (range: 219-268).

Most of the errors of the restricted dogs resulted from the fact that they continued to follow the path which had been correct in the test of the previous day, although the situation had changed.

Each day after the dogs had been tested in the maze, training in a simple motor habit was carried out. The animals were taken one at a time into a quiet room and taught to roll over, using a pellet of food as reward. On the first day 25 trials were given, with 10 trials per day subsequently. The restricted dogs took an average of 34.3 (range: 30-39) trials to learn the habit, the free-environment an average of 16 (range: 7-26).

The final problem used was the ramp test. A table 42 inches high and 30 inches square was placed in the centre of an enclosure which was 18 feet square. By running up a wooden ramp, or runway, the dog could reach the top of the table, where it was given food. The starting place was constant, and the ramp was moved through four positions corresponding to the four sides of the table. Each time that the ramp was moved, it was replaced by a false ramp, which was similar to the bottom half of the true ramp but did not reach to the table top. Soon after the restricted animals had been brought out of their cages, all the dogs had been trained to go up to the table for food with the ramp in position 1. One month later they were tested on all four positions, eight trials being given on each. The enclosure was marked off into squares, and an error was scored when the dog entered a square off the direct path to the correct ramp.

The mean error score for the restricted animals was 192.3 (range: 170-219), that for the free-environment 144.3 (range: 113-167). In addition, qualitative differences were observed. The restricted animals tended to persist in going to the position of the ramp which had previously been correct. They had a greater tendency than the free-environment dogs to run up and leap or fall off the end of the false ramp, behaving as if it were the correct one; and after the ramp had been moved to a new position they might sniff it occasionally, or take a few steps up, then leave it to run across the enclosure to one of the false ramps.

#### CONCLUSIONS

This work can be considered only a beginning in the analysis of the differences which exist between animals reared under different conditions. Even though the present data were obtained over a number of months, we do not know at present how permanent the observed differences are. Our methods of study are as yet crude, and we are not able to state the essential features underlying the singular behaviour of the

restricted animals. However, [these results clearly confirm the previous findings with rats in showing that animals reared under restricted conditions are inferior in problem-solving ability to those reared in a more complex environment. In addition, they suggest that marked disturbances of social behaviour and motivation may occur in the restricted animals.]

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## APPROACHES TO THE EXPERIMENTAL STUDY OF THE RORSCHACH TEST

GEORGE A. FERGUSON

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### INTRODUCTION

THE RORSCHACH TEST currently holds a unique position among tests used by psychologists in the clinical field. It is regarded by many as by far the most serviceable instrument as yet devised to explore and describe the personality characteristics of individuals. Training in the administration and interpretation of the Rorschach has become a prerequisite for employment in certain positions, and a class of specialist psychologist, known as a "Rorschach Worker," has apparently developed.

Many experimentally oriented psychologists are, however, critical of the test (10), and contend that the claims made by some Rorschach specialists are not supported by adequate evidence, that a cultist group has developed with its own terminology and an orientation unrelated to the experimental and theoretical approaches which have become generally acceptable in psychology. Questions of the kind, "Does the test measure what it purports to measure?" "What is known about the validity of the Rorschach?" and the like, are frequently raised, but, assuming that these are meaningful questions, answers acceptable to the experimentalist are not forthcoming.

Lee Cronbach, in a recent review of statistical methods applied to Rorschach scores (1), presents evidence to show that Rorschach workers have made many attempts to test their hypotheses using existing statistical methods. He presents a bibliography of seventy references, a large proportion of which describes experimental studies. He points out, however, that the use of faulty statistical procedures has in some cases led to unwarranted claims for significance, and in other cases to the rejection of significant relationships. Cronbach writes:

A few workers have been consistently sound in their statistical approach. But some of the most extensive studies and some of the most widely cited are riddled with fallacy. If these studies are to form part of the base for psychological science, the data must be reinterpreted. Perhaps ninety per cent of the conclusions so far published as a result of statistical Rorschach studies are unsubstantiated—not necessarily false but based on unsound analysis.

These findings suggest that many Rorschach workers lack the experimental and methodological skills necessary to test properly the hypotheses they formulate. This is not intended to imply criticism. Most Rorschach workers are employed in clinical situations and consequently

are required to appraise individuals for immediate diagnostic and therapeutic purposes as best they can with whatever instruments are available. Although they recognize the need for rigorous forms of evidence, they feel that the test has a certain *pragmatic validity* in clinical use. In adopting this view the Rorschach worker is clearly not alone. The justification for many diagnostic and therapeutic procedures in psychology, psychiatry, and in medical practice generally, depends on evidence of precisely this same class.

Since the Rorschach worker can hardly be expected to attain a high degree of sophistication in experimental methodology, statistical procedures, and the like, these not being his special fields of competence (largely concerned as he is with providing what he hopes and believes are useful answers to practical questions in the clinical setting), it follows that if the Rorschach test is to be put to a thoroughly critical appraisal it is clearly incumbent upon those who claim competence in experimental and methodological procedures to indicate how this might best be done.

Statistical studies of the Rorschach have frequently followed the pattern of relating Rorschach scores directly to criterion groups or scores. Conventional statistical procedures have been employed; mean or median test scores for different groups are calculated, significance of differences determined, and so on. A cursory review of studies in which such methods have been used suggests that this has been on the whole a sterile approach. Not much of a conclusive nature has resulted from studies employing methods such as these.

A current view is that such methods are inappropriate because their use results in a loss of information regarding the configurational properties of the data. The argument runs as follows. Statements made about individuals based on the use of the Rorschach test do not depend on the frequency or percentage of a given class of response but on the overall configuration of responses together with a qualitative interpretation of the protocol, observation of the behaviour of the subject during testing, and other items of information. It follows that a method which relates fragments of the response pattern to a criterion variable is inappropriate because information regarding the configurational properties of the data is lost. The inappropriateness of the commonly used statistical methods has frequently been noted. Robert R. Holt (5), in an article on statistical problems in clinical research, writes, "In the psychological experiment no one test score or clinical rating alone is crucial, nor can it stand for the total effect; the change in the configuration of such data must be analysed. But how does one test statistically

the significance of a difference in configuration?" And again, "It is taken for granted from the beginning in clinical research that the significant data are found in meaningful patterns, and that this patterning must be respected." Lee Cronbach (2), who has concerned himself with this problem and developed a method of pattern analysis, writes:

A major gap between psychometric and clinical methodology at the present time is the insistence of the clinician that the full meaning of the results of many tests can be understood only through study of interrelated patterns of scores. The conventional statistical methodology is unable to cope with patterns in a way approximating clinical pattern interpretation. The result is that research on many clinical problems and tests is incomplete or unsatisfactory.

If we accept the above lines of argument, and it appears we must, the question arises as to what methods can be used which are not subject to the criticism that information regarding the configurational properties of the data is lost. I propose to consider two possible approaches here. The first, referred to as the operational approach, stems from a formulation of certain logical aspects of the problem of validating the Rorschach test. The second, referred to as configurational analysis, is concerned with a statistical model of such a type that configurational properties of score patterns are preserved. It will be shown that such models already exist and that these may appropriately be used under certain limited conditions. It will be shown further, that these two approaches refer to two distinct problems; they do not refer to the same problem, as might initially be supposed.

#### THE OPERATIONAL APPROACH

If we are interested in the assessment of validity, that is, the relation between test results and a criterion for purposes of prediction, the Rorschach worker and the Rorschach test need not be regarded as distinct and separate entities but may be viewed as an inseparable combination. The question of validating the Rorschach test, as an entity in some sense distinct from the Rorschach worker, then ceases to be a meaningful question. The test-worker combination becomes an instrument in itself which can be validated as a whole in the same way that any other instrument is validated. This instrument when exposed to certain stimulating circumstances produces answers to certain problems. It makes statements about individuals. We may concern ourselves with the usefulness for practical purposes of these statements produced by our test-worker instrument, and with their relationship to statements about individuals made, so to speak, by other instruments. This approach implies that for the purposes we have in mind we need not concern

ourselves with the internal workings of the instrument, that is, with the complex processes whereby the test-worker combination arrives at the statements produced, any more than a statistician concerns himself with the complex processes whereby a calculating machine arrives at the answers to a problem when certain combinations of keys are pressed. Clearly, however, those who train Rorschach workers may be very much interested in this in much the same way that the technicians who construct calculating machines are interested in how the calculator works. For the purposes of assessing the predictive capacity of the test-worker combination, however, this aspect of the problem need not concern us. Of course, if the calculator yields answers to problems which are different from the answers reached by other means in which we have confidence, we may decide to discard the calculator or have it repaired. Likewise, if the test-worker instrument yields statements about individuals which depart grossly from statements reached by other means in which we have confidence, we may seriously doubt its usefulness, and may even discard it or at least search for ways to improve it.

It follows that the logic which is applicable in the general psychological testing field is applicable here. In intelligence testing the term intelligence may be assigned an operational meaning, in addition to other clearly distinguishable classes of meaning; that is, it may be defined operationally in terms of the responses of individuals in a defined test situation. At the operational level, "intelligence" as measured by one test is not of necessity the same thing as "intelligence" as measured by another test unless their equivalence can be operationally determined by appropriate methods. We may investigate the consistency with which a given test measures what it does measure. This, in general, is the problem of reliability. We may investigate the relationship between what the test measures and other attributes of behaviour, namely, those which we wish to predict and which are operationally defined by other means. This is the problem of validity.

In the case of the Rorschach, certain attributes of personality are operationally defined in terms of the functioning of the test-worker instrument; that is, the definitions which attach to these attributes are prescribed by the operations performed by that instrument. Statements about these attributes are what they are. The extent of agreement between statements made by different Rorschach workers when exposed to the same data may be the object of study. This is a concern for the reliability of our test-worker combination. The relationship between statements made about individuals by Rorschach workers and statements about the same individuals arrived at by other means—clinical case

histories and the like—may be the object of study. This is a concern for the validity of our test-worker combination.

Now the descriptions of personality produced by Rorschach workers are usually expressed in general qualitative terms. Likewise the criteria to which these descriptions are to be related are not infrequently expressed in general qualitative terms. The problem then arises of how best to describe the relationship between two sets of qualitative descriptions. This class of problem occurs fairly frequently in clinical research. A variety of approaches have been tried. Global diagnostic categories and the like may be used, and conventional statistical methods for expressing the relationship between two such sets of categories employed. Discrete responses to a set of specific questions may be used, and these in turn may be related singly or in combination to a set of criterion categories. Matching techniques may be tried, where judges are required to match one set of qualitative descriptions with another set. These are problems of detailed methodology which I do not propose to elaborate further here. I do not regard these problems as intractable.

My purpose has been to outline a certain logical approach which when grasped is exceedingly simple. This approach is generally applicable when we proceed to provide an answer to the question, "How reliable and how valid is this instrument in this situation?" The problem of statistical procedures which result in no loss of information regarding the configurational properties of Rorschach scores need not be raised in this connection. This problem is bypassed by the rationale of the approach.

None the less the problem of the internal workings of our test-worker instrument does arise. Here the concern is with the discovery of means by which the instrument may be made more reliable and more valid in a given situation, the question being, "By what process of tinkering, if any, can the validity and reliability of the statements made by the instrument be improved?"; that is, we are interested in the types of adjustments which may be made to our instrument in order that it may function more satisfactorily for the purposes we have in mind.

One approach to this problem involves the identification of particular score patterns which enable prediction of a criterion variable or discrimination between criterion groups. Such information would presumably facilitate the functioning of our instrument. It may be said to make that instrument more objective. I am not implying here that we should attempt to eliminate entirely the personal evaluation of the Rorschach worker, and thus make the technique entirely quantitative and mechanical. Probably this is neither necessary nor desirable. The Rorschach

worker uses and will continue to use, in arriving at the statements made, many sources of information which cannot be readily represented in any known and usable statistical model. I presume that a knowledge of particular configurations of scores of known predictive capacity will supplement and not replace these other scores in the generation of statements about individuals.

#### CONFIGURATIONAL ANALYSIS

The problem as it has been formulated by Cronbach (2) and others involves the development of a method by which sets of scores can be related to a criterion variable without loss of information regarding the configurational properties of the score pattern. I propose to deal with this problem in broad terms because, as far as I can gather, the solution either already exists or is indicated by what is already known, although this fact has seemingly been overlooked. Further, the discussion here is applicable to all situations where decisions are based on patterns of scores, and is not specific to the Rorschach.

A suggested line of approach involves the utilization of a multi-dimensional model. Consider an individual score pattern comprised of  $k$  variate values. This pattern can be represented as a point in  $k$  dimensional space with reference to  $k$  rectangular co-ordinates. The individual score pattern is then uniquely represented in a space of  $k$  dimensions. This model preserves the configurational properties of the pattern. Points for two individual patterns will coincide only in the case where the two patterns are identical. When given a criterion variable, we may examine the arrangement of these points in various regions of that criterion variable.

To illustrate, consider the simplest case where the score pattern consists of two variate values, and these are to be related to a criterion variable. Each individual pattern along with the criterion variate value may then be represented in a space of three dimensions. For practical purposes the variables may be grouped into class-intervals or categories. Let the number of class intervals for the criterion variable and the two test variables be  $l_0, l_1, l_2$ , respectively. The data then appear in the form of a trivariate frequency distribution containing  $l_0 \times l_1 \times l_2$  cells. This means that we are considering  $l_1 \times l_2$  possible score patterns, and the distribution of scores on the criterion variable is broken down into  $l_1 \times l_2$  component arrays. The mean criterion variable score for each of these  $l_1 \times l_2$  arrays may be calculated. A plane may then be fitted to this set of  $l_1 \times l_2$  means, if it is assumed that a plane is a good fit and the set of means does not possess unusual curved properties. This plane may

then be used to predict scores on the criterion variable for any individual score pattern.

Those who are familiar with statistical procedures will note immediately that this is a multiple regression model. If a plane is fitted to the  $l_1 \times l_2$  means, the equation of this plane is a multiple regression equation, and the regression weights are the parameters defining the location of the plane. If the score pattern is comprised of  $m$  variate values the problem becomes one of fitting a hyperplane to the  $l_1 \times l_2 \times \dots \times l_m$  array means and the equation of this hyperplane is a multiple regression equation.

The implication of the above is that the technique of multiple regression results in no loss of information regarding the configurational properties of the data, provided the regressions may be assumed to be linear. This conclusion is contrary to what has on occasion been thought.

Information regarding the configurational properties of the data is preserved in the multiple regression model by treating the variables two at a time in all possible combinations. The relationship between the score patterns and the criterion variable is described appropriately by the multiple correlation coefficient. Where the criterion is comprised of two groups only, a discriminant function may be employed.

In the case of the Rorschach, and possibly in the case of other tests where interpretations are based on patterns of scores, it may well be that the regression plane is curvilinear. Under these conditions the use of the ordinary technique of multiple regression may obscure real relationships which exist in the data. If the regression is thought to be curvilinear it is possible to fit multiple-regression curves by mathematical methods or by a method of successive approximations (3). Since the nature of the curvilinearity may be complex, and the fitting of such regression curves may involve much arithmetical labour, this approach may not prove useful in practice.

A more direct method is this. As previously, consider the test variables as grouped into  $l_1, l_2, \dots, l_m$  class intervals, an arrangement which admits of  $l_1 \times l_2 \times \dots \times l_m$  possible profiles. Calculate the mean criterion score directly for each profile, and then by inspection identify particular profiles or clusters of profiles which correspond to high or low scores on the criterion variable, as may be required. A variety of practical techniques can probably be devised to facilitate this inspection process. It is clear, of course, that where this method is used with more than a few variables, either a coarse grouping or a large number of cases may be necessary, otherwise the array means may be so unstable as to make impossible the drawing of definite conclusions. The overall relationship

between the score patterns and the criterion variable may be appropriately described by the multiple correlation ratio (6). This would involve the calculation of the variance of deviations about the  $l_1 \times l_2 \times \dots \times l_m$  array means, and the application of a modification of the usual correlation ratio formula. It may be necessary to apply certain corrections to the coefficient thus obtained (7, 8, 9).

If the criterion variable is dichotomous, the number and percentage of cases falling within each of the criterion groups may be calculated for each profile. Again, particular profiles or clusters of profiles which discriminate between the criterion groups may be identified by inspection. If it is thought necessary to describe in summary form the discrimination obtained, an appropriate statistic can probably be quite conveniently devised for this purpose.

The procedures discussed above are applicable generally to any situation where the concern is with the prediction of a criterion variable from a pattern of scores or with describing in some convenient way the relationship between the two. In the case of the Rorschach, special problems present themselves because, as the test is usually scored, three sub-sets of scores are obtained each of which adds up to  $R$ , the total number of responses. This may present a difficulty in detail which I have not studied at this time.

My purpose has been to indicate in broad terms lines of approach which might be adopted in dealing with data of the Rorschach type. In practice difficulties will arise, and special techniques will require development to deal with these difficulties as they occur. Clearly, however, existing statistical methods or extensions of those methods may appropriately be used in dealing with problems of this class. The difficulty inherent in many experimental studies of the Rorschach test does not reside in the lack of statistical models which have some degree of appropriateness to the problems at hand, but rather in our failure to use models which we already have and know, or elaborations of these models which can be readily attained.

#### CONCLUDING OBSERVATIONS

While the adequate evaluation of the Rorschach test, and of other techniques of this class, presents many methodological difficulties, it is clearly incumbent upon us to pursue these evaluative processes by the most rigorous methods available. This must be done, otherwise we are faced with the serious risk of perpetuating procedures which are worthless and a psychological orientation which is false. That this risk may

apply with the Rorschach is not entirely impossible, however unlikely it may appear to us at the present.

In this connection I am reminded of Franz Gall, a competent anatomist and an investigator of some scientific acumen for his time, although, owing largely to the actions of his follower Spurzheim, history has deemed him otherwise. Gall was interested in the problem of the relationship between the anatomy and physiology of the brain and human behaviour, one of the major issues in modern psychology. A recent book by D. O. Hebb (4) is devoted largely to this same problem. Gall's theory was that the development of the convolutions of the brain, which could be identified by an examination of the protrusions on the surface of the skull, was related to certain personality characteristics. He gathered some experimental evidence in support of his theory which to himself and his followers appeared convincing. Gall would probably have argued that his approach to personality had a certain pragmatic validity, in that within his experience of the cases investigated by him it seemed to work. Now Gall, as it turned out, was wrong. We may raise the question of how Gall, a capable and probably sincere investigator, was so grossly led astray. The answer to this resides in the fact that the terms used by Gall to describe personality—and, to a large extent, the terms used at the present time—frequently result in descriptions which appear to fit a multiplicity of individuals. Owing to their inability to describe with confidence the personality characteristics of themselves and others, many people will accept willingly almost any description which appears to rest on an allegedly objective technique, and will be convinced, on the basis of their experience, of the adequacy of such descriptions. Facts of this kind have been known and used by carnival fortune tellers for centuries. As psychologists, *circa* 1951, we of course believe that what happened to Gall's theories and techniques will not happen to our own, and that we are in some sense nearer to the truth than he. I feel, none the less, that we run the same risks; consequently, in order to minimize these risks, we must subject our theories and techniques to the most rigorous scrutiny possible. This must be done; otherwise we relinquish any claims we may have to scientific integrity.

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## BLOOD PRESSURE RESPONSE TO REPEATED BRIEF STRESS IN PSYCHONEUROSIS: A STUDY OF ADAPTATION<sup>1</sup>

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A NUMBER OF studies from this Laboratory (3, 4, 5) have suggested the hypothesis that psychoneurosis is characterized by defects in physiological regulation of reactions to stress. Defective physiological regulation may show itself in two main aspects of reaction to stress: (1) in increased *magnitude* of the immediate reaction, and (2) in persistence of the reaction at high level with continuation or repetition of stress (low degree of *adaptation*<sup>3</sup>). In the previous studies cited, the experimental design was focused mainly on the first of these (magnitude of reaction). The present study was designed primarily to investigate adaptation in arterial blood pressure. In accordance with our hypothesis, we should predict less adaptation to repeated stress in psychoneurotic patients than in normal control subjects.

In a recent review of the literature dealing with blood pressure in psychoneurosis (3), it appeared that investigations of blood pressure *level* failed to provide conclusive evidence of deviation from normal. Malmo and Shagass found that psychoneurotics differed from controls chiefly in their longer continuation of reaction to stress. In performing a psychomotor test, patients and controls showed about the same initial rise in blood pressure, but the patients' rise continued longer.

The present study reports a follow-up experiment in which several psychomotor tests were given, permitting a more adequate investigation of the adaptation phenomenon than was possible in the previous study (3).

<sup>1</sup>This paper was presented at the Tenth Annual Meeting of the Canadian Psychological Association, University of Western Ontario, London, Ontario, May 28-30, 1951.

This research was carried out under contract No. DA-49-007-MD-70 between the Office of the Surgeon General, U.S. Department of the Army, and McGill University.

<sup>2</sup>The authors are indebted to Mr. A. K. Bartoshuk for his assistance in statistical analysis of the data, and to Dr. J. F. Davis and Mr. R. E. Quilter for their technical advice and assistance.

<sup>3</sup>We mean by adaptation *the decrement in a response which is a consequence of its repeated elicitation*. The term, as we have defined it in this paper, should not be confused with the use of this word in the connotation of "adjustment."

## METHODS

### *Subjects*

Subjects were 22 psychoneurotics, 10 males and 12 females, ranging in age from 18 to 61 years, with a mean age of 38.4 years; and 20 controls, 10 males and 10 females, ranging in age from 18 to 41 years, with a mean age of 24.4 years.\* Diagnoses in the patient group were as follows: anxiety state, 9 cases; anxiety hysteria, 3 cases; reactive depression with anxiety, 3 cases; reactive depression, 2 cases; conversion hysteria, 2 cases; mixed psychoneurosis, 2 cases; anorexia nervosa, 1 case.

### *Apparatus*

Auscultatory blood pressure measurements were made with a standard mercury sphygmomanometer. Diastolic blood pressure was read at the point of disappearance of the sound. Blood pressure recordings from the left arm were made with the Cambridge Recording Sphygmotonograph developed by Lange (2). Our checks on the reliability and validity of this instrument made in the instance of the previous experiment (3) and repeated in the present instance have shown that it is adequate for the purposes of our experiments.

On the average, about six readings of either diastolic or systolic pressure may be taken each minute with this machine. It is impossible to record both diastolic and systolic simultaneously.

### *Procedure*

The procedure is outlined in Table I. The subject was brought into the experimental room, and following a brief rest, manometric readings were taken. Then, by means of the recording sphygmotonograph, three-minute continuous tracings of both diastolic and systolic blood pressure were recorded, with the subject relaxed. The diastolic tracing was taken first, the systolic tracing second, with a one-minute period intervening.

After a rest period of two minutes, administration of the stress tests commenced. The order of presentation is given in Table I. The duration of the entire session was about 90 minutes. The material for the Circle Test consisted of a printed circle,  $2\frac{1}{4}$  inches in diameter. On the side nearest the subject there were two dots, one inside, the other outside the circle at a distance from it of  $1/8$  inch. These dots were the starting

\*It should be mentioned that, although ideally it would have been preferable to have attempted to conceal identity of the subjects from the experimenter (e.g. which ones were patients and which controls), this was not feasible. However, the examiner (R. M. H.) attempted at all times to maintain the same reassuring, encouraging attitude toward all subjects.

points for the two circles which the subject was required to draw, successively.

The paper-and-pencil Maze of Porteus (6) for age XI was used as the second psychomotor test. In order to reduce practice effect in conditions 3 and 6, the Maze was inverted. Each test, Circle and Maze, was performed first without mirror, and subsequently under the usual conditions of mirror drawing (that is, with his direct vision blocked, the subject is required to proceed with drawing while he views paper and pencil in a

TABLE I  
OUTLINE OF PROCEDURE

A. RESTING CONDITIONS		MACHINE RECORDING*
1. Manometric Readings		
2. Machine <sup>o</sup> Diastolic—3 min. (Rest—1 min.)		
3. Machine Systolic—3 min. (Rest—2 min.)		
B. STRESS CONDITIONS		MACHINE RECORDING*
Performed without mirror	1. Trace Circle	Systolic
	2. Porteus Maze XI	Diastolic
	3. Porteus Maze XI (inverted)	Systolic
Performed with mirror	4. Trace Circle	Systolic
	5. Porteus Maze XI	Diastolic
	6. Porteus Maze XI (inverted)	Systolic
	7. Cold Pressor 1	Systolic
	8. Cold Pressor 2	Systolic
	9. Cold Pressor 3	Systolic
	C. RESTING CONDITION	
Repeat Manometric Readings		

\*Cambridge Recording Sphygmotonograph

mirror). Diastolic recording was made for Maze only, first without the mirror and second with the mirror (see Table I). The cold pressor test was carried out in the standard way (1) with an immersion time in ice water of 60 seconds. Only systolic blood pressure was recorded during the three cold pressor tests.

Because it was not possible to record both systolic and diastolic blood pressure simultaneously with the Lange instrument, it was necessary—in this study—to concentrate our data-taking on either systolic or diastolic blood pressure. Largely on the basis of results from a previous experiment (3) in which systolic blood pressure appeared more differ-

entiating than diastolic, we chose to concentrate on systolic recording, but not to omit diastolic recording altogether.

Each stress test was divided into four separate periods. These intra-test intervals were as follows: (1) *Pre-period*. Preceding instructions there was a 45-second rest during which the "pre-period" tracing was made. (2) *Instruction period*. During the instruction period for the psychomotor tests, the subject took pencil in hand, placed the pencil on the starting point, and listened to the instructions. Exactly 60 seconds were allowed for this. The same interval was provided for instructions in the cold pressor tests. (3) *Stress period*. Performance (or immersion) commenced at the end of this 60-second period. Performance time varied with the subject's speed in completing the task. Immersion time for cold pressor was exactly 60 seconds for all subjects. (4) *Post-period*. Following performance (or immersion) there was a 90-second rest during which the "post-period" tracing was made. The sphygmotonograph cuff was deflated between tests in order to avoid excessive discomfort from partial arrest of circulation in the arm. Deflation periods were one minute following conditions 2 and 5 (diastolic recording), two minutes following conditions 1, 3, 4, and 6 (systolic recording), and three minutes following conditions 7 and 8 (cold pressor tests).

#### *Treatment of Data*

Each point at which blood pressure was recorded on the tracing was measured. The measurements were averaged for each subject, with means computed for the four parts of each of the nine stress tests. These individual means were employed to determine group averages. In evaluating tests of statistical significance, the conventional criteria were employed.

The data were also analysed to determine the influence of age and sex. Age did not appear to affect the findings. Those sex differences which were statistically reliable will be presented.

## RESULTS

#### *General Effect of Test Situation*

Auscultatory blood pressure readings for each patient were taken on the ward under relatively non-stressful conditions on five separate days during the week of the experiment. The mean values for these ward readings were 118.1/72.9, reliably lower than the mean auscultatory values of 124.9/78.3 obtained just prior to testing, before the exact nature of the tests had been disclosed to the subjects. A similar comparison was, of course, not possible in the case of control subjects for

whom no basal readings were available. These data from the patients are in line with the expectation that merely facing a task (prior to gaining information concerning its exact nature) leads to apprehension, accompanied by activation of the sympathetic division of the autonomic nervous system.

TABLE II  
BLOOD PRESSURE LEVELS BEFORE AND AFTER STRESS TESTS

	Before tests			After tests	
	Auscultatory	Machine			
		Rest	Pre-l (Systolic)		
Patients	124.9/78.3	127.2/81.6	126.5	121.5/81.9	
Controls	120.3/76.5	120.2/80.0	120.1	114.8/77.0	

Mean blood pressure levels obtained before and after the stress series are shown in Table II. The average levels for both patients and controls remained quite steady throughout the first ten minutes or so. Systolic levels in the pre-period of the first test were almost identical with the pre-test auscultatory values. The differences between mean blood pressure levels of patient and control groups were not statistically reliable.

Table II also shows that systolic pressure tended to be lower after testing than before, in both groups, and that diastolic pressure tended to rise in the patient group; these tendencies proved reliable statistically. The data indicate a trend in both groups towards adaptation of systolic reaction under continued stress, with greater adaptation in the controls. On the other hand diastolic pressure showed the reverse of adaptation in the patients, while it remained steady in the controls.

#### *Systolic Blood Pressure*

Average systolic values for the four parts of each test are plotted separately for controls and patients in Figure 1.

(a) *Intra-test changes.* In each of the single tests, note the blood pressure rise from the pre-period to the instruction period, the rise from instructions to performance (or immersion of the hand in ice water), the fall in blood pressure from this active period to the post-period, and the continued fall in the pre-period, preceding the next test. It will be

noted that these intra-test changes were very similar in both controls and patients. The consistency of the response pattern in individual tests may be judged from the fact that blood-pressure rise during performance

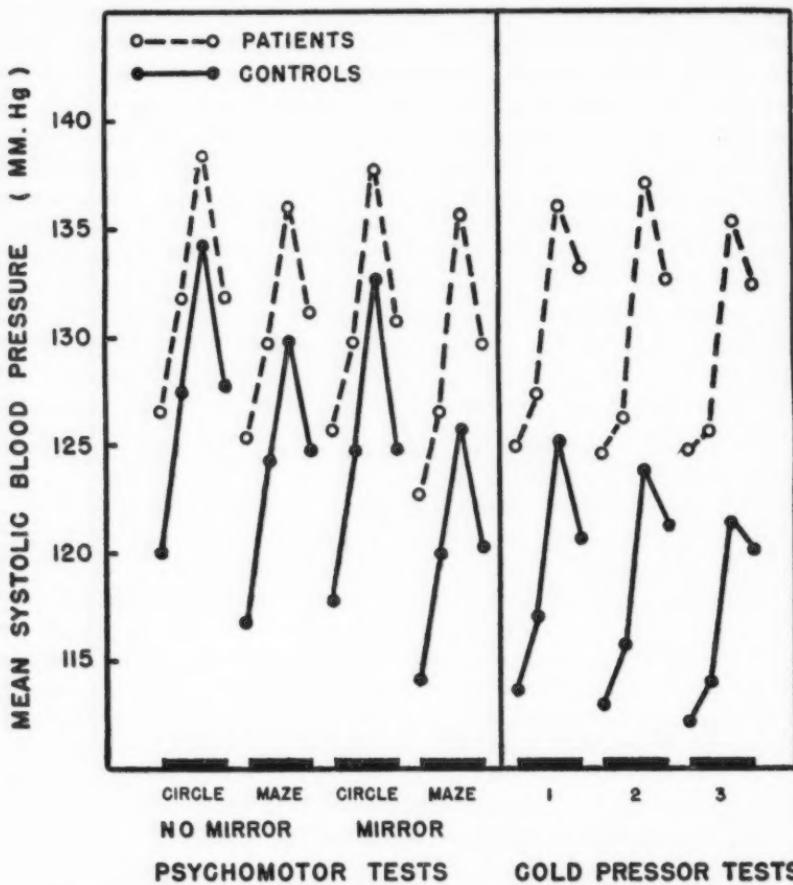


FIGURE 1. Mean systolic blood pressure during each of the four intra-test periods: pre-period, instructions, performance (or immersion), and post-period. (Points are arranged in chronological order.)

was found in 92 per cent of all individual tests. Comparison of patients and controls with respect to intra-test changes yielded only two statistically reliable differences. The rise from the last third of the instruction period to the last fourth of the performance period for the

Maze Mirror Test was reliably greater for patients than controls (mean rise of 10.6 mm. compared with 5.9 mm.). Also the rise from the end of the instruction period to the final 20 seconds of immersion in the third cold pressor test was greater in the patients (mean rise of 15.2 mm. compared with 10.2 mm.).

(b) *Inter-test changes.* Viewing the time course of the session as a whole revealed a significant difference between patients and controls. Note that the lower curve in Figure 1 (for controls) tends downward more than the upper curve (for patients). The inter-test differences between patients and controls are shown more clearly in Figure 2 which presents the data from the psychomotor tests, plotted separately for each period. Note that in every instance the controls showed greater adaptation than patients. Analysis of variance applied to data from all four periods showed that this difference between patients and controls was, statistically, very significant.

There is one other feature of these curves which merits attention: the break in regularity from the second point to the third. Note in the control graph for performance how the blood pressure rose when the mirror was introduced (making the drawing test more difficult and hence more stressful). A similar rise may be seen in the curve for patients. The breaks in the curves for pre-period and instruction period are probably due to the fact that just prior to the pre-period for the first mirror drawing test, each subject had been given a brief period of practice on mirror drawing, and thus anticipated the stress of mirror drawing. These rises are an indication of the sensitivity of blood pressure to increased stress.

Comparison of the systolic reactions on the same performance tasks with and without the introduction of the mirror showed that the mirror increased the magnitude of reaction. The increase was statistically reliable in both patient and control groups for the maze. It should also be noted that systolic rise was reliably greater in the patients for the maze with mirror.

The *immediate* reaction of controls to the additional stress presented by the mirror was as great, if not greater, than that of patients. The difference between patients and controls appeared on the second mirror drawing test in which the controls' reaction to *repetition* of stress was lower than that of the patients. The overall curves for the three cold pressor tests (Figure 1) reveal that here also the controls showed greater adaptation than the patients.

Fractionation of the data according to sex revealed a sex difference (see Figure 3). Male subjects showed greater adaptation in blood

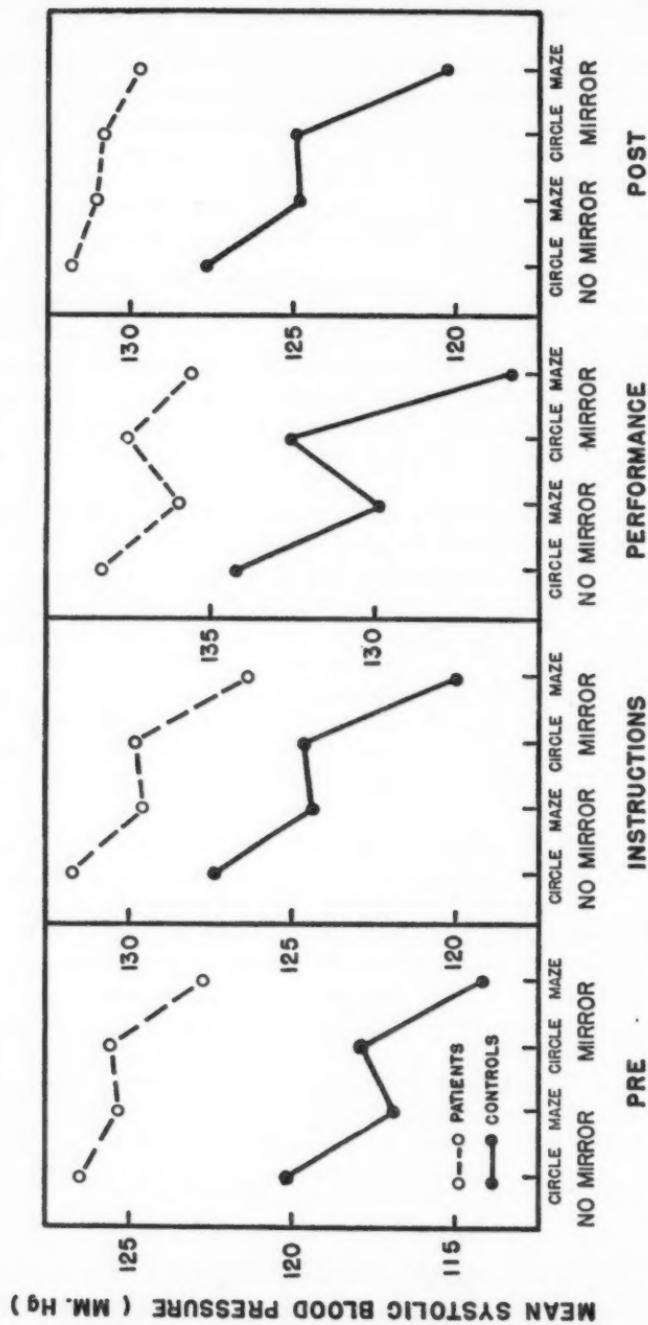


FIGURE 2. Mean systolic blood pressure in psychomotor tests, plotted separately for each intra-test period. Note that controls show greater adaptation than patients, especially in performance and post-period.

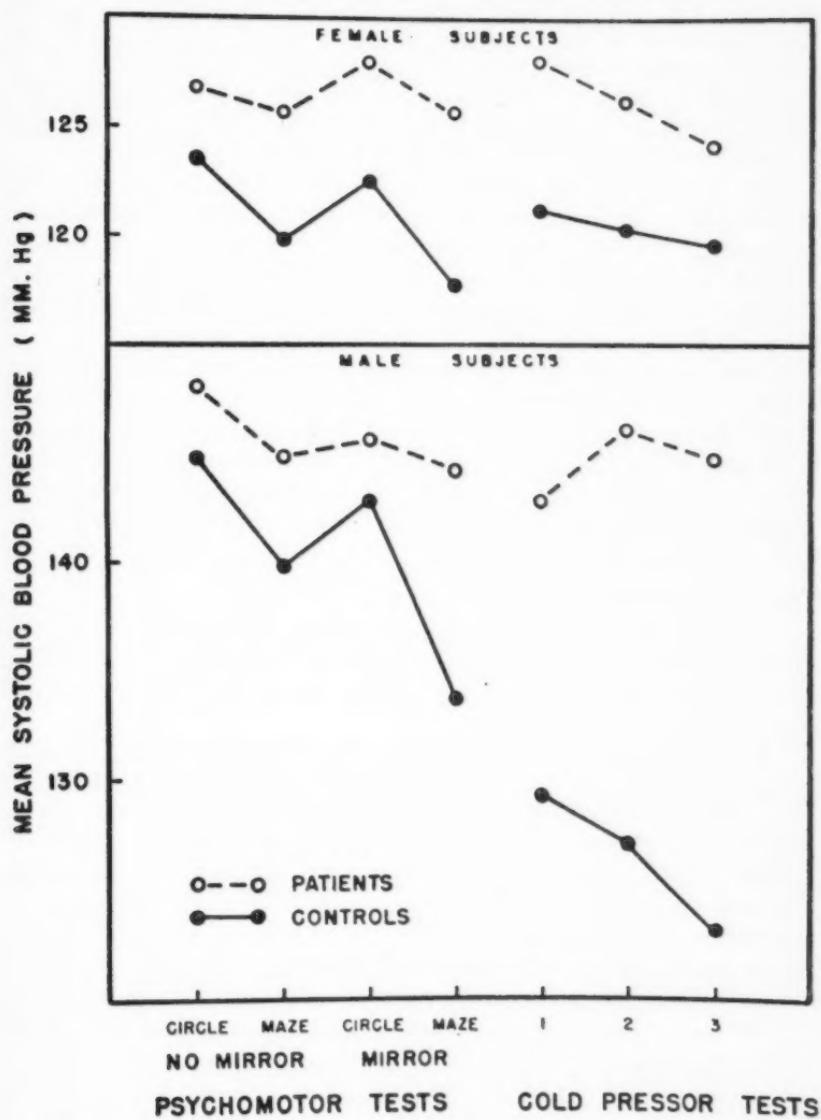


FIGURE 3. Comparison of male and female subjects with respect to adaptation in systolic blood pressure during four successive periods of performance in psychomotor tests and three successive periods of immersion in cold pressor tests. Note that male controls show greatest adaptation.

pressure than female subjects. Analysis of variance applied to the data from normal controls showed that the sex difference was reliable for the cold pressor series, but not for the psychomotor series. The curves shown in Figure 3 were plotted from the data taken during the performance periods of the psychomotor tests, and during the immersion part of the cold pressor tests. The sex difference is clearly evident.

On the average, the patients took longer to complete the tests than the controls. This raised the question whether the factor of performance time, itself, was the main one responsible for the observed patient-control differences in systolic reaction. However, further statistical analyses showed that performance time did not influence the blood pressure results significantly.

Coefficients of correlation between performance times and each of the following variables were computed: (1) rise in systolic blood pressure from instruction period to performance, (2) fall in systolic blood pressure from performance to post-period, and (3) an "adaptation measure" (fall in systolic blood pressure from the performance period of the first psychomotor task to that of the last). None of these correlation coefficients was statistically significant.

#### *Diastolic Blood Pressure*

Diastolic pressure was recorded during the three-minute "rest" period and during only two stress tests, both involving maze tracing, the first time without the mirror and the second time with the mirror (tests 2 and 5).

Following test 1, in which systolic pressure was recorded, diastolic pressure tended to fall significantly in both patients and controls. Mean pre-period values for test 2 were 73.3 and 78.0 for controls and patients respectively, compared with means of 80.0 and 81.6 for the "rest" period. This constitutes some indication for early adaptation of diastolic reaction which did not seem to persist with continuation of stress.

Figure 4 shows curves for diastolic blood pressure which are similar, in the way they rise and fall, to those for systolic blood pressure (compare with Figure 1). It will be noted that, as in the case of the curves for systolic blood pressure, there is greater separation of curves for patients and controls, *later* in the series. Both groups showed a rise in diastolic blood pressure from test 2 to test 5. This was probably due to the fact that the later task was more difficult, and hence more stressful. But in the patients, this increase was slightly greater than it was in the case of the controls.

Fractionation of the data according to sex, in so far as this could be done with the limited data for diastolic blood pressure, revealed one

reliable sex difference. The male subjects, particularly male controls, showed greater rises in test 5 than the female subjects. These findings with diastolic blood pressure must be considered as merely suggestive. The most significant finding at present appears to lie in the fact that intra-test diastolic changes under stress tended to parallel systolic changes.

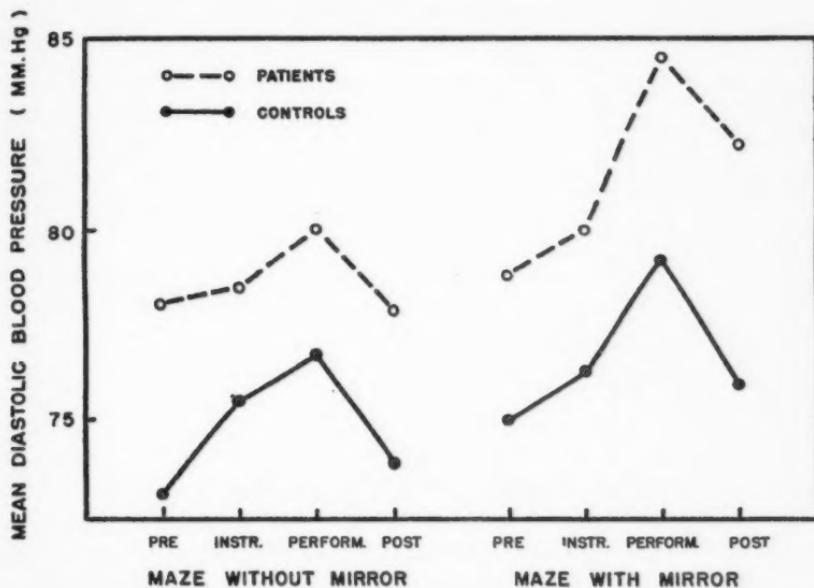


FIGURE 4. Mean diastolic blood pressure during each of the four intra-test periods of Porteus Maze Test, performed first without mirror, and later with mirror.

#### DISCUSSION

The main finding of the present study may be stated as follows: under the same conditions of imposed non-specific stress, systolic blood pressure in controls showed greater adaptation than in patients. Pressor reactions to stress were found in both controls and patients at the outset of testing. Further, each brief stress test evoked a similar pattern of pressor reaction in both groups. However, as the session progressed, the controls' reactions tended to diminish and the patients' reactions tended to persist at the initial level.

In the design of this experiment the stress tests were arranged in order of what was assumed, on subjective grounds, to be increasing

degree of stress: performance tasks without mirror, performance tasks with mirror, cold pressor stimulation. The intra-test blood pressure changes observed during performance and immersion periods coincided with our "common-sense" evaluation of the relative stressfulness of the tests, being greatest for the cold pressor and least for the tasks performed without mirror. In this instance, blood pressure reaction proved to be a good objective indicator of degree of stress as judged subjectively.

Full interpretation of the significance of impaired adaptation of blood pressure reaction to stress in psychoneurosis must await the results of further experiments. Present data do support the formulation, made on the basis of previous findings, that psychoneurosis is characterized by deficiency in regulatory mechanisms which normally operate to check excessive physiological reaction. We have obtained evidence that deficient regulation may be observed in both skeletal and autonomic systems (3, 4, 5). While we have found that responses of the skeletal musculature in psychoneurosis differ from normal both in terms of increased magnitude and diminished adaptation, the dimension of magnitude seems to be more striking there, and certainly more so than is the case with blood pressure.

Our present data provided only two instances of greater intra-test blood pressure reaction in patients. These occurred in the last psychomotor test and in the last cold pressor test, and consequently may be interpreted as manifestations of differences in adaptation. One may consider that reactions of the skeletal muscles represent rapid, relatively brief changes in which the possibilities for rapid alterations in tension are practically unlimited. On the other hand, blood pressure represents a supportive function, in which changes are relatively limited. In view of these considerations, it is not surprising to find the critical psychoneurotic deviations to be, in magnitude of skeletal-motor reactions, and in slower return to base-line in autonomic reactions, like blood pressure.<sup>5</sup>

Clinical observations of pathologically increased emotional response in psychoneuroses find objective confirmation in quantitative physiological studies such as the present one. However, it should be pointed out that such confirmation is of limited value unless it leads to new concepts which pave the way for clearer definition of the nature of these disorders. The concept of defective physiological regulation seems to be

<sup>5</sup>It should be pointed out that a broader picture of cardiovascular function, provided by additional measures such as cardiac output and peripheral blood flow, would have been very desirable. Conclusions are necessarily limited when blood pressure is recorded alone. However, the need for recording physiological reactions *while the subject is actively engaged* makes it impossible to use procedures which have been developed for use only under resting conditions.

useful in this respect. Detailed knowledge about the nature of these deficiencies should lead to the discovery of those regulatory mechanisms in the central nervous system whose action is defective in psychoneurosis.

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## BOOK REVIEWS

*General Psychotherapy—Dynamics and Procedures.* By D. EWEN CAMERON. Toronto: The Ryerson Press, 1950. Pp. 312. \$6.00.

THIS is a serious attempt to present to the professionally interested reader a systematic account of psychotherapy. Dr. Cameron's approach is eclectic and comprehensive. He describes in detail the various kinds of psychotherapy. The methods range from the permissive and essentially non-directive "depth" therapy (Integrative Psychotherapy), through various forms of the directive method including the use of hypnosis, to re-education, group therapy, and nursing psychotherapy. The procedures are well illustrated by reference to clinical material, much of it taken from actual recordings of psychotherapeutic interviews with patients.

Not the least valuable part of this book is the first two chapters in which Dr. Cameron sets forth his basic assumptions and skilfully builds his theoretical structure from which, in turn, he develops logically his methods in psychotherapy.

In a work such as this, semantics is naturally important, and in order that his concepts will be equally understood by all the disciplines interested in mental health, the author has endeavoured to write in words which are understood and have the same meanings for the social scientists as well as the psychiatrists. In this he has been reasonably successful and as a result the free exchange of ideas and information on psychopathology and psychodynamics should be materially helped by this book. Nevertheless the text is not exactly light reading. The occasional introduction of specially coined words (diploidal and syncytial) were temporary obstacles to this reviewer.

Dr. Cameron's views on the training of the psychotherapist are of considerable interest. For the physician interested in general psychotherapy he firmly believes that a personal analysis has several disadvantages. In this he is in direct disagreement with the psychoanalytic group. Another point of disagreement with current psychoanalytic practice is his recommendation that the psychotherapist should conduct a physical examination of his patient before proceeding with therapy. He feels this establishes the therapist in the mind of his patient as a "real doctor." Some of the other reasons favouring this procedure may be challenged. For instance, the presence of a kyphosis (a marked forward slump of the back and shoulders) in a woman "suggests that there may have been considerable sex shame during puberty."

This book will be especially useful to graduate students in psychiatry and to those advanced students in psychology who have an interest in

clinical and counselling methods. There is a good bibliography after each chapter, and an index.

J. D. GRIFFIN

*Canadian Mental Health Association  
Toronto*

*Experimental Design in Psychological Research.* By ALLEN L. EDWARDS.  
Toronto: Clarke Irwin & Co., Ltd. 1950. Pp. 446. \$5.00.

How OFTEN have teachers of statistics been harassed by students who in their eagerness to complete the requirements for master's or doctor's degrees pay little or no attention to planning their experimental work, and who appeal for help only when they realize, often too late, that serious, sometimes insurmountable, problems of statistical analysis become the lot of those who fail to collect their data in a certain way and for a specific purpose! Such teachers will welcome the appearance of Edwards' new book *Experimental Design in Psychological Research*. This book should do much to reinforce for the practical psychologist the vital principle, first suggested by Professor R. A. Fisher, that "the actual and physical conduct of an experiment must govern the statistical procedure of its interpretation." Canadian university students in psychology and related fields appear to be just at the point where they are becoming more conscious of the need for carefully designed and well controlled experiments. So, for Canadian statistics teachers at least, this book, in which the author elaborates the principles of experimental design and illustrates in essentially non-mathematical language how these principles may be applied in psychological research, is most acceptable.

Edwards begins his book by classifying the different types of variables which occur in problems of psychological research. In Chapter II, which contains an introduction to some important principles of experimental design, and in Chapter III, which shows how probability is related to experimental design, the author follows closely the pattern set by Professor R. A. Fisher in *The Design of Experiments*. Edwards, however, reduces his explanations to a more elementary level and uses psychological rather than agricultural examples. The application of the normal and chi-square distributions to research problems, testing hypotheses about correlation coefficients, and the use of the *t*-test in the statistical analysis of small sample data are discussed in Chapters IV through IX. The last eight chapters of the book are devoted to the analysis of variance and the analysis of co-variance as these techniques are related to simple and more complicated designs in experimental

psychological research. In his treatment of the analyses of variance and co-variance Edwards has done for psychological research what Lindquist did for educational research in *Statistical Analysis in Educational Research*, "to point out specifically and illustrate concretely what seem to be the most promising applications of Fisher's methods."

*Experimental Design in Psychological Research* is set up in very readable type. At the end of each chapter are examples giving sets of data, for the most part obtained in actual experiments, which students may use to test certain hypotheses. Those who work through these exercises can scarcely fail to get a good grasp of the methods presented in the chapters. Answers to all examples are given in the appendix.

In his book, Edwards does not limit himself to a discussion of principles of experimental design in psychological research. He says in the Preface that "this book attempts to present to the student in psychology, education, sociology, and the behavioral sciences, some of the newer developments in statistical analysis, particularly with respect to small-sample theory, as they relate to problems of research and experimentation in these fields." In fact, a large part of the contents of the book consists of a treatment of methods of statistical analysis. In view of this, some might wonder why the words "statistical analysis" were not used in the title of the book. It may be that by omitting these words Edwards is implying that "experimental design" is a term which, when correctly used, pertains to a broad field which includes statistical analysis. Or possibly the title, *Experimental Design in Psychological Research*, was used to clearly distinguish this book from his earlier book, *Statistical Analysis for Students in Psychology and Education*.

Edwards presents little that is highly original; clearly the book is a secondary source of information on statistical analysis and experimental design. The author quite rightly acknowledges that he owes much to the various publications of R. A. Fisher, Frank Yates, John Wishart, M. S. Bartlett, G. W. Snedecor, W. G. Cochran, and others. Even so, it is the opinion of this reviewer that Edwards has made a worthwhile contribution. He has written for the student who is not mathematically inclined, and in a style that should be intelligible to such a student and yet not objectionable to the mathematician. A great service is done by one who "hands down" to students of statistics the principles of modern statistical developments so that even with a modicum of mathematics they can understand them and apply them properly. Edwards is particularly good at this. He can explain how the binomial is related to the normal distribution without leaving the reader with the feeling of awed mystification which some mathematical writers seem to

love to engender. This is an art which should receive due recognition. Even though we admit that the role of the originator is most important, we should also remember that, at least as far as students are concerned, the part played by the "communicator" is essential.

Teachers and students alike should find *Experimental Design in Psychological Research* useful. Instructors in psychology departments may use it as a text for a half-year or full-year course in experimental design. It could be used to advantage as a general reference book by almost any beginning research student who is "with thesis."

H. M. FOWLER

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*University of Toronto*

*The Psychology of Dictatorship.* By G. M. GILBERT. New York: The Ronald Press Co., 1950. Pp. viii, 327. \$4.00.

*The Psychology of Dictatorship* is a psychological interpretation of "the interplay of personalities and social process" by the prison psychologist of the Nuremberg trials. From his unique vantage point Gilbert had access to the evidence presented during the trials of the war criminals, was free to test and interview them personally, and to observe their behaviour pending and during the trials. Around data gathered over a year's period he builds his latest book. Other documentary sources and data from the Wechsler and projective techniques are liberally cited and a bibliography of sixty-four items appended. The book falls into three major divisions: Part I, The Genesis of Dictatorship—A Psychocultural View; Part II, Selected Case Studies; Part III, Psychological Implications.

Part I contributes little of an original nature but provides an historical framework and defines the thesis of the book:

It is our thesis that authoritarian leadership like any other, reflects the nature of the culture in which it emerges. This is expressed first of all, in the social values developed among the potential leaders of the given culture. Case histories would necessarily reveal the authoritarian influences in their upbringing and the cultural channeling of their aggressions. . . .

. . . We shall approach the study of dictatorship through an examination of the personalities in the constellation of leadership in Nazi Germany. This constellation . . . breaks down into three principal identification groups or group-clusters that appear characteristic of such movements. . . .

The last half of Part I concerns Hitler. Hitler was, of necessity, excluded from the case studies because direct interview material and observation were at that date unavailable. This section of the interpretation is therefore based upon secondary sources.

The case studies are composed of the three "identification groups":

1. the revolutionary nucleus—Hermann Goering, Rudolph Hess, Hans Frank
2. the "political bandwagon"—Franz von Papen, Joachim von Ribbentrop
3. the agencies of aggression—Field Marshall Keitel, SS-Colonel Hoess.

This selection of cases was determined "by a desire to present a well-rounded picture of the revolutionary nucleus as personalities developing around Hitler," and the study, as Gilbert points out, is handicapped by the survival capacities of the group. A clear, composite picture of each leader is achieved: Goering, the egotistical psychopath who identified himself with the national heroes of the German history books; Rudolph Hess, whose recurrent amnesia and pseudoparanoia caused the Tribunal to request an examination into his mental state and who as he awaited the death sentence "feverishly divested himself of a thousand-page essay on Hitler to aid the writer in analyzing the mysterious character who had made him do what he did"; von Ribbentrop whose complete collapse prompts Gilbert to say, "All attempts to get at his basic convictions proved fruitless for he had no basic convictions."

Already criticism has been levelled at the book on the grounds that Gilbert sets out to interpret within a social psychological framework and instead speaks like a psychoanalyst. Neither part of this statement is wholly true. Gilbert set out to interpret the interaction of personalities and social processes. True, he draws largely upon psychoanalytic theory and concepts in discussing what he designates "the psychopathic group," but even here developmental history, cultural and historical influences are much in evidence. In dealing with the behaviour of the other groups he is right in asserting

Unlike the revolutionists, the men . . . did not do so out of any pathological need for outlets for aggression; they suffered from no paranoid ego-involvement nor did they follow Hitler in tormented response to libidinal urges. To understand their motives we need no devious excursions into depth psychology. Their motivation is quite apparent on the level of social values, group identifications and aspirations—and plain political opportunism.

Gilbert criticizes various schools in their analyses of leadership. The economic determinism of the Marxists, the overemphasis on dynamics of personality of the psychoanalytic school, and the "Group mind" of sociologists and social psychologists alike fall short of adequately explaining the facts. While he concedes the influence of the "Zeitgeist," he appears to agree with Hitler's Chief of Operations, General Jodl, "In

this war, the absolute guilt rests with one man only—Adolf Hitler." Gilbert suggests that the "traits of leadership" drawn up in the past by American psychologists are in sad need of revision: "The results have yielded what amounts to interesting trait-clusters of the American academic ego-ideal . . . these appear somewhat whimsical as a yardstick of leadership in the present study." In the Nazi microcosm quite different traits were the determinants.

Gilbert's book slips smoothly into the historical pattern. He cites no evidence contradictory to documentary facts. One might wish for further access to the referenced private manuscripts and war crimes records, unavailable to the public and therefore immune to criticism here. The interpretation, however, is so logical that it is difficult to see if or where the data may be coloured by the author's own biases. Again, as an American he doubtless interprets Nazi personality by American standards; the reviewer belongs to the same cultural group and finds it difficult to distinguish the cultural biases.

Gilbert writes well and on an interesting topic. His organization is good. These combine to produce fascinating as well as informative reading and we might venture to predict that the book will be widely read, especially by the layman. Again we might wish—whimsically, it is true—that similar studies were available *before* rather than after a world war.

PATRICIA ANNE SOLBERG

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*Culture and Personality*. Edited by STANSFELD SARGENT and MARIAN W. SMITH. New York: Viking Fund, 1949. Pp. 219.

THIS REPORT of the "proceedings of an Interdisciplinary Conference" held in 1947 is outstandingly characterized, as are most such reports, by a critical discrepancy between what the reader experiences and what the participant experiences. Despite careful editing, the primary impact on the reader is of a rather disjointed talking past one another on the part of the authors, even in the reporting of the discussion among them, where, presumably, they are trying to minimize formality and maximize communication. All this despite—or because of—the galaxy of stars who are the authors of the several papers: Fromm, Gardner Murphy, David Bidney, Kardiner, Kluckhohn, George Herzog, L. M. Hanks, Jr., Klineberg, Komarovsky and Sargent, Linton, Harry Stack Sullivan, and Henry A. Murray. And again this despite the orderly arrangement of topics: I, Culture and Personality—Defining Our Terms; II, Techniques for Studying Culture and Personality; III, Evaluation of Studies, and IV,

Integration for Future Studies. One cannot help but feel—and hope—that the conference itself was more orderly, more unified, and more productive of mutual understanding than the published report suggests.

The lack of unity makes it impossible to comment on the volume as a whole, except to note that it establishes rather clearly the little distance we have come and the great distance we have to go to establish a useful framework within which to study personality-in-culture or culture-in-personality. We all agree that they are empirically inseparable. We mostly agree that they have been usefully separated analytically. We nearly all feel that the present pressing problem is to devise an analytic scheme in which these analytic elements will have their relation clarified. And we mostly get no further than to declare our faith that "These things must be...."

A number of the essays, taken severally, would repay careful study. The most notable, in the reviewer's judgment, is Ralph Linton's contribution on "Problems of Status Personality." The suggestiveness and importance of what he has to say stand in direct relationship to the simplicity and clarity of his language. The same relationship holds—but with opposite consequences—for the contribution of the late Harry Stack Sullivan on the "Multidisciplined Coordination of Interpersonal Data." His essay lends sharp point to Henry Murray's plea that we should first attempt to clear the fog by using one term in one sense to cover one agreed-on analytic element. The reviewer agrees with him that we are not each likely to know what the other is talking about until we do so. The reviewer would add that he is not sure we each know what we are talking about in a field in which every man's language is to a very large degree a private language.

JOHN R. SEELEY

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*Personality in Peptic Ulcer.* By ALBERT J. SULLIVAN and THOMAS E. MCKELL. Springfield, Illinois: Charles G. Thomas, 1950. Pp. x, 100. \$3.50.

THIS LITTLE BOOK, written by gastroenterologists, was intended mainly for practising physicians. The authors, who have treated more than 1000 cases of peptic ulcer, offer "a theory of the production of peptic ulcer on the basis of multiple etiologic factors." Peptic ulcer is produced not by one etiological factor but rather by a combination of many factors varying in their respective importance in each case. The factors are: (a) constitutional (ulcer habitus: slender, narrow costal angle, scaphoid abdomen); (b) predisposing personality (traits of "typical ulcer person-

ality": restless, active, driving, versatile, successful, responsible, determined, conscientious, over-extended); (c) acute precipitating emotional situations; (d) trauma which may be internal (food, condiments, or alcohol), external (a violent blow on the upper part of the abdomen), or intrinsic (gastrospasm); (x) all the physiological and pathological factors which are unique in producing an ulcer rather than some other disorder. The book is chiefly devoted to a discussion of factors *b* and *c*.

The 200 most thoroughly studied cases were classified as follows: Group A (72 per cent of cases)—“typical ulcer personality”; Group B (11 per cent)—definitely psychoneurotic; Group C (5 per cent)—strong external precipitating situation (factor *c*); Group D (10 per cent)—factors *b* and *c* minimal, but some unusual degree of factors *a*, *d*, or *x*; unclassified (2 per cent).

The simplicity and unpretentiousness of this little book are refreshing, and as a quite impressionistic account of personality factors in “the typical ulcer patient” by physicians who have seen 1000 cases of a common psychosomatic disorder, this book should be of interest to psychologists.

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1952 ANNUAL MEETING  
CANADIAN PSYCHOLOGICAL ASSOCIATION

*Place:* Banff School of Fine Arts, Banff, Alberta.

*Time:* June 16, 17, 18.

*Accommodation:* The Banff School of Fine Arts has a number of chalets in which double rooms or dormitory accommodation may be obtained. The Special Convention Rate to members of the CPA is \$5.00-\$6.00 per day including meals.

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<i>From</i>	<i>To Banff</i>	<i>To Lake Louise</i>	<i>To Vancouver</i>
Toronto .....	\$132.05	\$133.45	\$147.33
Ottawa .....	141.05	142.45	165.08
Montreal .....	154.80	156.00	171.28

*Papers:* Members are asked to note that no paper should exceed fifteen minutes in presentation time. Student Affiliates may present papers if sponsored by a Full Member or Fellow of the Association. The *Programme Committee* welcome suggestions for Round Table discussions, symposia, etc.

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